

THURSDAY, JANUARY 28, 1892.

THE ASTRONOMICAL THEORY OF THE
GLACIAL PERIOD.

The Cause of an Ice Age. By Sir Robert Ball, Astronomer-Royal for Ireland. Pp. 180. (London: Kegan Paul and Co., 1891.)

THIS book gives a popular account of the theory of Adhémar and Croll as to the causes of glacial periods in geological history.

The author's power as a popular expositor is well known, and this little book shows him at his best. He knows when to drive a point home, and yet is never tedious in his reiteration. But he has given here something more than a lucid explanation, for he makes a valuable contribution to the subject, and the book may be read with advantage by those who are already acquainted with the literature bearing on the theory.

The theory itself may be sketched in outline as follows:—

It is known that, under the perturbations of Venus and Jupiter, the eccentricity of the earth's orbit varies within certain limits. When the eccentricity is large, and when the precession of the equinoxes brings the perihelion to near the middle of, say, the northern winter, the annual supply of solar heat is so distributed that there will be a glacial period in the northern and a mild climate in the southern hemisphere. Two or three maxima of glaciation and mildness will usually succeed one another at intervals of 10,500 years, because the eccentricity varies with extreme slowness. When the eccentricity is small, as at present, a moderate climate will prevail in both hemispheres, whatever be the position of the perihelion.

The keynote of Sir Robert Ball's presentation of this theory is given in a short mathematical appendix. I am disposed to dissent to some extent from the manner in which this view is set forth, but the general argument will, I think, do much to convince the scientific world of the truth of the theory, even where Croll's more elaborate discussions failed to do so.

I will now give a paraphrase of the argument, and will point out where it appears to me open to objection.

The time taken by the earth to describe a degree of longitude round the sun varies as the square of its distance from the sun, and the intensity of solar radiation varies inversely as the square of the same distance. Hence the amount of heat received by the whole earth during the description of a degree of longitude is constant.

Let the year be divided into only two seasons, viz. the northern summer or southern winter when the sun is north of the line, and the northern winter or southern summer when the sun is south of the line. Also let similar days in summer and winter be defined as days on which the sun sets (say at Greenwich) as much after 6 p.m. as before 6 p.m.; similar parts of summer and winter will mean parts limited by similar days.

Now consider the solar heat incident on any specified area of one hemisphere, during any specified portion of the summer and during the similar portion of the winter. Suppose that the heat incident on the area in the portion

of summer added to that incident on it during the similar portion of winter be denoted by 2, and suppose that the excess of the heat incident in the portion of summer above that incident in the similar portion of winter be denoted by $2a$; then it is clear that $1 + a$ is proportional to the amount of heat received by the specified area during the specified portion of its summer, and $1 - a$ is proportional to the amount of heat received by the area during the similar portion of winter.¹ Thus we may say that the contrast between the summer and winter supplies of heat (for given area and given portions of summer and winter) is represented by the fraction $(1 + a) \div (1 - a)$.

This is, of course, equally true when the whole hemisphere, and the whole of summer and winter, are considered, and Sir Robert Ball shows that a is then equal to $2 \sin 23^\circ 27' \div \pi$; $(1 + a) \div (1 - a)$ is found to be almost exactly as 5 to 3. Using percentages he gives the ratio as 63 to 37, but the simple numbers 5 to 3 afford a closer approximation to accuracy.

It is clear that if the specified portions of summer and winter embrace the solstices, and if the specified area is tropical, a will be small, and if it is polar it will be large. The fraction $(1 + a) \div (1 - a)$ continually increases as we go northward, and it may be taken as a measure of the severity of a climate. It is quite uncertain how far the climate of any one place depends on the heat supplies of the whole hemisphere on which it lies, and therefore it is uncertain how large an area and how long a season we ought to take into consideration in the present investigation. But I should have thought it legitimate, in treating of the causes of glaciation, only to consider the semi-annual heat supply of a polar cap, comprising, say, all the area north of latitude 30° ; thus would have made $(1 + a) \div (1 - a)$ much greater than 5 to 3. It does not seem to me, however, that we are bound to find an answer to this almost insoluble problem.

So far we have considered the supply of heat whilst the earth describes so many degrees of longitude round the sun, but climate depends on the supply of heat during a given time.

When the earth's orbit is circular, summer and winter are of equal length, and so also are similar portions of summer and winter; thus the two ways of estimating the heat supply coalesce, and the contrast between the summer and winter *daily* supplies of heat is also represented by the fraction $(1 + a) \div (1 - a)$. The present condition of affairs differs but little from this standard case, and we know that the contrast between the summer and winter daily supplies of heat is such as to produce certain known climates, differing according to latitude.

¹ If $\frac{1}{2}\pi \pm \phi$ be the sun's hour angle at sunset on any day in summer, and on the corresponding day in winter, and if the sun's parallax on those days be proportional to $\pm E$, then it is easy to show that the amount of heat received by unit area in the course of the day is proportional to

$$(1 \pm E)^2 (\phi + \cot \phi) \pm \frac{1}{2}\pi \sin \delta \sin \lambda,$$

where $\pm \delta$ is the sun's declination, $+$ in summer and $-$ in winter, and λ is the latitude of the place of observation.

It follows that, what is called in the text, the contrast for unit area in latitude λ , for this pair of days is—

$$\frac{1 + a}{1 - a} = \frac{(1 + E)^2}{(1 - E)^2}, \text{ where } a = \frac{\pi}{2(\phi + \cot \phi)}.$$

The expression for the heat supply on unit area during any portion of summer or winter involves elliptic integrals, which might be given if it were worth while.

A triple integral is required to express the heat supply of any specified area during any specified portion of the year.

The question we have to ask is, If the orbit becomes eccentric, how will the contrast of daily supplies be affected?

In order to answer this, let us go at once to the extreme, when the eccentricity of orbit is a maximum. We learn that if aphelion is at midsummer, summer will be 199 days, and winter 166 days; and the converse is true when the perihelion is at midsummer.

Since 199 is to 166 nearly as 6 to 5, we see that with midsummer perihelion there are 5 days of summer to 6 of winter, and with midsummer aphelion there are 6 of summer to 5 of winter.

Hence, with midsummer perihelion, the short summer daily supply of heat may be taken as proportional to $\frac{1}{5}(1+a)$, and the long winter daily supply as proportional to $\frac{1}{6}(1-a)$. Hence the contrast between the short summer and long winter daily supplies is represented by $\frac{6(1+a)}{5(1-a)}$; that is to say, the standard contrast

is augmented in the ratio of 6 to 5. Next, with midsummer aphelion, the long summer daily supply of heat may be taken as proportional to $\frac{1}{6}(1+a)$, and the short winter daily supply as proportional to $\frac{1}{5}(1-a)$. Hence the contrast between the long summer and short winter daily supplies is represented by $\frac{5(1+a)}{6(1-a)}$; that is to say,

the standard contrast is diminished in the ratio of 5 to 6.

In the first case, the heat supply is less evenly distributed through the year than at present, and we have a much more severe climate; in the second, it is more evenly distributed, and we have a much milder one. It follows also that, if we compare the two extreme cases together instead of both with the mean case, the change of contrast is represented by the ratio of 6^2 to 5^2 , or of 36 to 25.

I must refer the reader to the able discussion in the book of the effects which we have reason to suppose would flow from a change of contrast represented by the numbers 36 to 25; and it must suffice to say here that it seems enough to explain on the one hand the occurrence of the glaciation of England, and on the other hand the occurrence of sub-tropical plants in Greenland.

Now, the above seems to me to be substantially the argument in the book, but I dissent from the stress laid on the numerical determination of the quantity a . On p. 90 Sir Robert says:—

“This theory will be entirely misunderstood unless the facts signified by these numbers (the evaluation of $(1+a) \div (1-a)$) are borne in mind. No one can discuss the astronomical theory of the Ice Age unless the figures 63 and 37 (5 and 3 are more accurate) form a portion of his consciousness, and the refrain of his every argument.”

It may be admitted that it might have been more difficult to present the argument in a popular form without assigning a numerical value to a , but Sir Robert Ball is fully equal to such a task; and I contend that the numerical value of a is beside the mark, even if a value, appropriate to the investigation in hand, were attainable.

After presenting his own view of the question, Sir Robert Ball says (p. 134) that Croll does not seem to have been really aware of the full strength of the astronomical theory, and in this I entirely agree. Croll, in fact, rather weakens than strengthens his position when

he tries to trace in detail the action and reaction of the astronomical cause, for in doing so he is led to maintain various theses which are not susceptible of proof, and are even highly doubtful. He thus takes as the central point of his position one at which it appears to me to be weakest. In 1886 I wrote:—

“Adequate criticism of Mr. Croll’s views is a matter of great difficulty, on account of the diversity of causes which are said to co-operate in the glaciation. In the case of an effect arising from a number of causes, each of which contributes its share, it is obvious that if the amount of each cause and of each effect is largely conjectural, the uncertainty of the total result is by no means to be measured by the uncertainty of each item, but is enormously augmented. Without going far into details, it may be said that these various concurrent causes result in one fundamental proposition with regard to climate, which must be regarded as the keystone of the whole argument. That proposition amounts to this—that climate is unstable.

“Mr. Croll holds that the various causes of change of climate operate *inter se* in such a way as to augment their several efficiencies. Thus, the trade-winds are driven by the difference of temperature between the frigid and torrid zones, and if from the astronomical cause the northern hemisphere becomes cooler, the trade-winds on that hemisphere encroach on those of the other, and the part of the warm oceanic current, which formerly flowed into the cold north zone, will be diverted into the southern hemisphere.¹ Thus the cold of the northern hemisphere is augmented, and this in its turn displaces the trade-winds further, and this again acts on the ocean currents, and so on; and this is neither more nor less than instability.

“But, if climate be unstable, and if from some of those temporary causes, for which no reasons can as yet be assigned, there occurs a short period of cold, then surely some even infinitesimal portion of the second link in the chain of causation must exist; and this should proceed, as in the first case, to augment the departure from the original condition, and the climate must change.”²

I see no reason to depart from what I said five years ago, but I now learn from this book how it is that Croll mistook the strong points of his own theory, and that a more forcible proof of it may be contained in a short work than in an elaborate volume. After expressing this opinion, it is but fair to quote and indorse the following passage (p. 112) on Croll’s famous work on “Climate and Time”:—

“I was greatly struck,” says the author, “by this work when I first read it many years ago. Subsequent acquaintance with this volume . . . has only increased my respect for the author’s scientific sagacity, and my admiration for the patience and the skill with which he has collected and marshalled the evidence for the theory that he has urged so forcibly.”

There are a few other points in the “Ice Age,” not involved in the main line of argument, on which I should like to comment.

The method adopted of stating the disturbing forces of the planets on the earth appears to me unduly sensational. We learn (p. 74) that the disturbing force of Venus is 130 million million tons, and it is impossible not to be impressed with the magnitude of the force. But if we had been told that the disturbing force on each pound of the earth’s mass was only 1/7000 of a grain,

¹ Ball (p. 134) fails to see the force of this argument.

² Brit. Assoc. Report, 1886, Address to Section A.

we should have been equally impressed with its insignificance—and yet the two statements are virtually the same. In fact, the unscientific reader is not likely to realize the prodigious number of pounds in the earth's mass.

It may be remembered that Croll computes, in "Climate and Time," the value of the eccentricity of the earth's orbit from Leverrier's formulæ, and endeavours thus to assign actual dates to various glacial periods. Now, Sir Robert Ball very justly will not admit that our knowledge of the solar system is accurate enough to justify the application of these formulæ to the enormously long intervals of time involved. I think, however, that it would have been of interest to the general reader to be told in round numbers the kind of intervals which we have reason to believe may have elapsed between one glacial period and the next; in fact, to learn whether the intervals are probably millions of millions of years, or hundreds of thousands of years. I conjecture that our knowledge of the planetary movements is sufficient to enable us to say that such an interval may be something comparable with 200,000 years. I should like, further, also to ask Sir Robert Ball whether he does not consider that Leverrier's formulæ may probably be relied on to give at least a rough approximation for about 100,000 years in the past; and, if this is so, whether we might not conclude, with fair probability, that the last glacial period occurred about that number of years ago? I must, however, disclaim any special knowledge on this point, and I should gladly see his opinion, or that of any other physical astronomer, on the matter.

In conclusion, I wish to say that, in making the foregoing criticisms and suggestions, I have no intention of disparaging the book; on the contrary, it is only because it is a good book that it is worth while to consider it carefully. I have found it profoundly interesting from end to end, and I am convinced that it will be widely read, as it deserves to be.

G. H. DARWIN.

POPULAR ZOOLOGY.

Animal Sketches. By C. Lloyd Morgan, F.G.S., Principal of University College, Bristol. (London: Edward Arnold.)

THIS is one of those delightful books of natural history for young people which their parents never had the benefit of, and for which they ought to be duly thankful. A competent naturalist here gives them the result of his full and varied knowledge, but gives it so blended with imagination and humour, so intermingled with anecdote and personal adventure or observation, as to make it a real story-book about animals, by reading which we learn much of their lives and habits, their peculiarities of structure and their relations to each other, while we seem to be only reading for amusement. There is nothing systematic in this volume. It is merely a collection of miscellaneous chapters on a variety of animals, beginning with the lion and ending with the oyster, every chapter of which is both pleasant and instructive.

The best way to notice a book of this kind is to give a few examples of the author's style, which in this case will

certainly commend the book better than any description of its contents. First, then, as a bit of serious biology, we will give a passage on the nesting-habits of the ostrich.

"The nest is scooped out in the sand, and two or three hen-birds may combine to lay their eggs in it, to the number of about twenty. It is said, and that by several observers, that, besides the eggs laid in the nest, each hen lays several in the neighbourhood, and that these are broken when the young are hatched, and the contents are given them as food. But I am inclined to regard these statements with some suspicion. The hens take turns in sitting during the day, never leaving them long in the scorching heat of the South African sun. But at sun-down the cock-bird takes charge of the eggs, and sits throughout the night. He is not going to be bound by any conventional rules as to the proper division of labour between the sexes.

"A very careful observer, Mrs. Barber, has drawn attention to the fact that the indistinct grey colours of the hen ostrich are wonderfully adapted for purposes of concealment. These birds while upon their nests do not erect their necks, but place them at full length in front of them upon the ground; and the grey-brown body might, Mrs. Barber says, be easily mistaken for some other object, such as, for instance, an ant-hill, so common on the plains of South Africa. That so large a bird should be inconspicuous may seem surprising; but another observer, Mr. W. Larden, tells us of his experience with the rhea, or South American ostrich, which seems quite to bear this out. 'One day,' he says, 'I came across a rhea in a nest that it had made in the dry weeds and grass. Its wings and feathers were loosely arranged, and looked not unlike a heap of dry grass; at any rate the bird did not attract my attention until I was close on him. The long neck was stretched out close along the ground, the crest feathers were flattened, and an appalling hiss greeted my approach. It was a pardonable mistake if for a moment I thought I had come across a huge snake, and sprang back hastily under this impression.'

"The male ostrich, with his splendid black and white feathers, would not be thus inconspicuous *by day*. But he sits at night, and his strength and pugnacity would induce most other creatures to let him alone. Mrs. Barber describes the careful manner in which the female bird approaches the nest in the morning, when her turn for incubation has come. In wide circles, and apparently in the most unconcerned manner, she will feed round the nest, never once looking towards it, but gradually approaching nearer and nearer to it by diminishing each circle as she walks round, until at length her perambulations have brought her to within a yard or so of the nest, when the birds will rapidly change places, the male walking swiftly away, and not remaining in the vicinity of the nest during the day. The wonderful rapidity with which the change is effected is perfectly astonishing, and it is impossible to see the exact manner in which it is done, so swiftly do they change places."

As an example of Mr. Lloyd Morgan's lighter manner, what can be more attractive than the opening sentences of his chapter entitled "Long-nose, Long-neck, and Stumpy"?

"And which of all the animals in the Zoo do you like best?" I said to a bright, fair-haired little girl whom I had assisted in her descent from the elephant.

"I think I like Long-nose, Long-neck, and Stumpy best, because they are so big and curious, and Long-nose best of all because he has given me a ride. Did *you* know it was his nose?"

"Of course I affected the most extreme surprise and